MAY 1956
VOL. V No. 3

TAXON

Official News Bulletin of the International Association for Plant Taxonomy, Edited and Published for I.A.P.T. by the International Bureau for Plant Taxonomy and Nomenclature, 106 Lange Nieuwstraat, Utrecht, Netherlands

HOMONYMS, NOMINA NUDA, AND THE DOUGLAS FIR.

by

R. Ross (London)

In the February number of this journal there appeared an article by de Wit entitled "Nomina nuda are not illegitimate!" (Taxon 5 : 4-7. 1956) in which the author very rightly points out that unless a name is validly published, its effective publication is not sufficient to make the later use of the name for another plant illegitimate. He states that certain authors have erroneously taken the opposite viewpoint and proposes certain amendments to the Code, including a definition of nomina nuda, to obviate future misinterpretations of this sort. As an example of incorrect practice in this matter he cites Franco's replacement of *Pseudotsuga taxifolia* (Poir.) Britt. by *Pseudotsuga menziesii* (Mirb.) Franco. There are further notes by Stafleu on this latter question in the February and April numbers (Taxon 5 : 19, 38. 1956).

These papers raise three issues, on all of which I wish to comment: firstly the desirability of amending the Code to make misinterpretations of this type less possible, secondly whether nomina nuda need defining in the Code, and thirdly the correct name of the Douglas Fir. In making my comments I shall have to quote the Code at times and refer to it often. When I do so I shall take advantage of the fact that, as a member of the editorial committee, I have in my hands a copy of the final draft of the new edition of the Code, and will use its words and the numbers of the articles and recommendations in it, giving the equivalents of the latter in the Stockholm Code. It will be seen that, although the quotations bear signs of the re-arrangements we have considered desirable, the wording is virtually unaltered.

In his paper de Wit does not specifically mention the article which, in conjunction with the homonym rule itself, definitely settles the question of what can be an earlier homonym; this is the article on definitions. The two relevant articles read as follows:

Art. 6 (Art. 10 of the Stockholm Code)

"....

"Valid publication is publication in accordance with Arts. 32-45 and C 24.

"A legitimate name or epithet is one that is in accordance with the rules.

"An illegitimate name or epithet is one that is contrary to the rules.

"....

"Note. In this Code, unless otherwise indicated, the word 'name' means a name which has been validly published, whether it is legitimate or illegitimate."

Art. 64 (includes Art. 74 of the Stockholm Code)

"A name is illegitimate in the following cases:

"....

"(2) If it is a later homonym, that is if it duplicates a *name* [my italics] previously and validly published for a taxon of the same rank based on a different

WEALTH MYCOLOGICAL INSTITUTE
30 APR 1956

type. Even if the earlier homonym is illegitimate, or is generally treated as a synonym on taxonomic grounds, the later homonym must be rejected."

The note to Art. 6 makes it plain that a name whose publication does not comply with the conditions for valid publication is not a "name" for the purposes of the definitions of legitimate and illegitimate names in the article, nor is it included within the meaning of the word "name" italicised above in Art. 64 (2); not only is there no indication that its meaning there differs from that given in the note to Art. 6, but the words "and validly" give a definite indication that its meaning is the same. I have, I realise, the advantage over many of my colleagues that one of the languages in which the Code is written is my mother tongue, but it seems to me that it could scarcely be clearer that a name cannot act as an earlier homonym unless it is validly published. I am convinced that any action to make it clearer must be by way of amending Art. 64 (2) itself. In considering de Wit's actual proposal, it must be borne in mind that names not validly published are not necessarily nomina nuda; they may for instance have been published as synonyms. It might perhaps be an improvement to begin the second sentence of Art. 64 (2) thus: "Even if this previously and validly published name....", but such a re-iteration seems hardly necessary. De Wit's raising of the question in this journal will of itself have been of great value in making the possibility of this misinterpretation considerably less.

The inclusion of a definition of a nomen nudum in the Code is advocated by de Wit on more general grounds, however. It is not necessary as the Code stands at present; the phrase appears only twice, in an example to Art. 42 (Art. 51 of the Stockholm Code) and in Rec. 45 C (Rec 54 D of the Stockholm Code). In the first of these it can be eliminated by saying "because the generic name *Phlyctidia* was not validly published" if its undefined use is thought to cause possible misunderstanding. In the second it ought to be eliminated, for this recommendation, which reads:

"Authors should avoid adoption of a name or an epithet which has been previously published as a *nomen nudum* for a different taxon."

ought to apply also to all other names or epithets not validly published, and especially to such as those invalid only because published since 1935 with a diagnosis in a modern language. I have only realised this whilst preparing the present note. Such a change seems to me within the powers of the editorial committee, and I have therefore proposed to my colleagues that this recommendation should read ".... previously but not validly published for ....". I hope it will be possible to make this amendment in the new edition.

De Wit's point, however, is not so much that the words "nomen nudum" in the Code need definition to make it intelligible but that help is needed to decide what constitutes a "description" for the purposes of Art. 32 (Art. 42 of the Stockholm Code). He contends that there is a tendency to regard as a description for the purposes of this article a few words of no real diagnostic value, and that this tendency is undesirable. His view is that sometimes name changes due to the discovery of overlooked synonyms would be avoided if what he calls nomina semi-nuda could be treated as invalid because not accompanied by a description. His proposal for achieving this is that a published statement that the words accompanying a name are without diagnostic value and the name therefore invalid should be treated like the selection of a lectotype and not set aside without a strong case to the contrary. He discounts the possibility of defining a description, but there is a criterion which can be used. It is whether or not the author's words were an attempt at a taxonomic definition of the species. If they were, they constitute a description. De Wit's proposed new note comes near to saying this, but it would in my view be better if it defined a description in the way I have just done. This seems to me too early a stage to table amendments for

consideration at Montréal, however, and I am therefore not putting forward a form of words at present. Whether de Wit's procedure for dealing with the residue of doubtful cases will aid stability in nomenclature is a matter for the Special Committee on Stabilization, and they will no doubt give it serious consideration.

There is little to add to Stafleu's notes (Taxon 5 : 19, 38. 1956) on the name of the Douglas Fir, in the second of which he rightly states that its correct name is *Pseudotsuga menziesii* (Mirb.) Franco, except to record that consultation of the original publications confirms that *Abies taxifolia* Desfontaines (Tabl. Ecole Bot. Mus. Hist. Nat., 216. 1804) was validly published. Desfontaines' work consists of a list of the latin and french names of the plants cultivated in the gardens and greenhouses of the Paris Museum. On p. 216 we find, after a list of species under the generic name *Pinus:*

"Abies (*Pinus* L.) Sapin
taxifolia
. . argenté. F. ♄ (écon.)"
pinus picea *L.*"

This is the style of citing earlier synonyms adopted by Desfontaines throughout the work, and, by the standards which are applied by established custom to works published before 1953, it is a reference to the previously and effectively published description provided by Linnaeus under the name *Pinus Picea* in Sp. Pl., ed. 1, 1001. 1753. Desfontaines' name is admittedly illegitimate because superfluous when published, but that reference makes it validly published, and hence *Abies taxifolia* Poir. (in Lam., Encycl. Méth., Bot. 6 : 523. 1804 [1805]) is a later homonym and illegitimate.

Another point brought out by Franco in his original note on this subject (Bol. Soc. Broter. 24 : 74-77. 1950), but not mentioned in either of Stafleu's notes is that *Abies menziesii* Mirb. (in Mém. Mus. Hist. Nat. Paris, 13 : 63, 70. 1825. "Menziezii") is a nomenclatural synonym of *Abies taxifolia* Poir., being founded, like Poiret's name, on *Pinus taxifolia* Lamb. non Salisb.

Gleason's contention (Rhodora, 57 : 332-5. 1955) that the species should be called *Pseudotsuga mucronata* (Raf.) Sudw. is based on lack of knowledge of Mirbel's publication and faulty interpretation of the rules, and is not correct.

In conclusion, I should like to thank Mr. Dandy for advice and assistance, and Dr. de Wit for the courteous and amiable way in which he discussed a draft of this paper with me.

PSEUDOTSUGA AND PSEUDO-SCIENCE

by

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In reprinting the essence of Boivin's Latin article on *Pseudotsuga taxifolia* and *P. menziesii* (Taxon 5 : 19. 1956), there was overlooked a later article by Franco (Bol. Soc. Broter. II. 28 : 115-116. 1954)* in which Boivin's views were refuted, as well as a cryptically titled article by Gleason (who also did not see Franco's later article) stating still another but equally erroneous conclusion ("Pedanticism run amuck," Rhodora 57 : 332-335. 1955). Briefly, Franco states that *Abies taxifolia* Desf. (1804)

*) See also Taxon 5 : 38. 1956.

was valid because reference was made to *Pinus picea* L. and its accompanying description, thus making illegitimate *Abies taxifolia* Poir. (1805), based on a different type. Using the first legitimate epithet (earlier than Rafinesque's *mucronata,* upheld by Gleason), Franco supplied the necessary and correct binomial, *Pseudotsuga menziesii* (Mirbel) Franco.

But much more serious than the correct binomial and the legal details which uphold it are the actions, attitudes, and principles exemplified by the resolution presented to the "Special Committee on Stabilization" by the Society of American Foresters (Taxon 5 : 18. 1956), and more especially the inflammatory article published earlier by W. H. Camp (Taxonomic Index 17 : 45-48. 1954), brought in as point of departure for the resolution. When first I read Dr. Camp's article, I was left with mixed feelings of astonishment and exasperation. Now that an organized group is attempting to exploit his name, position, and publication, there seems a real need to speak out against what is becoming a headlong flight away from established scientific procedure.

I take sharp issue with Dr. Camp's remarks on the codification of botanical and zoological names for use on machine-sorted cards. An illustrative precedent for such action already exists in systems for the classification of library books. Nobody has insisted that biologists must fossilize their knowledge into some rigid and unchanging scheme for the benefit of librarians and users of libraries. Nobody has issued decrees to require taxonomists to grovel hat in hand for permission to use this or that name. The classification adopted in the Dewey Decimal System ("Relativ Index"), followed by a majority of U.S. libraries, is abominable. It corresponds to no accepted taxonomic system, but is a grotesque hodge-podge of several. I know of no taxonomist who follows it. Neither do I know of any librarian, manufacturer, politician, or bureaucrat who has tried to force botanists into accepting it in place of all others, or rewrite their Code of Nomenclature to fit it.

One cannot codify data for a machine unless one first has all the data. We do not know everything about even a tiny fraction of the plant kingdom. Improving our present knowledge is a tedious, difficult, and time-consuming process. Let us freely admit that we live in an imperfect world and in an imperfect time. But let us not get into a psychological stew about it, and above all let us not hand ourselves over self-roped and tied to outsiders who have no conception of what taxonomic biology involves or of how complex it is. If business men or inventors think they can do some good with the kind of machine proposed, they are perfectly free to try it. Doubtless they could easily devise something to handle a limited number of economically important species, or the common plants of limited areas. Library schemes could be adapted for such purposes. But only the very unrealistic would imagine that any such gadget is feasible for all or even a major portion of the world's flora. And all the world's flora is what codes of nomenclature and taxonomic methods must be able to deal with. Therefore let us not have irresponsible demands that we squeeze our science into limits that cannot possibly contain it.

Dr. Camp makes the hyperbolic statement that we "will be forced into the ludicrous position of having to go, hat in hand, to a bureau operated by non-taxonomists to ascertain what name should be put on a large number of organisms." What nonsense! We go to them? It is they who must come to us, for our knowledge is immeasurably greater than theirs, and our field of action is immeasurably larger. By such remarks Dr. Camp altogether disqualifies himself as an acceptable spokesman for the science he is supposed to represent. One would accept and respect the opinions of a man with an established record of exhaustive investigations, both monographic and nomenclatural. Dr. Camp is rather noted for his efforts to be stimulating. Perhaps he purposely exaggerates in order to produce action. I would prefer to see a more

considered and penetrating discussion. Reading his machine-code diatribe, one looks in vain for any calm assembling and weighing of background facts, any clear statement of purposes, any fair evaluation of methods. It reminds one of the speech of a political rabble-rouser. And its most evident consequence has been the kind sought by a ward-heeling politician: an uncritical group has been stampeded into proclaiming their support of it. This most emphatically is not science and is not scholarship.

Both the article and the subsequent resolution seek to reverse completely the established procedure in scientific work. We assemble and organize our knowledge according to methods and rules which have proved themselves impartial and effective. Only when we have carried objective possibilities to the limit do we admit expediency or considerations from outside the realm of pure science. Dr. Camp proclaims that the last and conditional resort must be granted precedence over the rest. This is a pernicious doctrine. With fervor and force I denounce it. Talk of "practical" needs is so much window-dressing. There is no excuse for shoving it into the forefront of scientific work.

The resolution published in Taxon is not a contribution to science or scholarship. It comprises a series of steps in political demagoguery. It begins by alleging (not detailing nor proving) an objectionable state of affairs, which in comparison with past conditions is certainly highly exaggerated. It bolsters this with inflated references to Dr. Camp and his official connections, an attempt to obtain agreement by a show of authoritative backing. Then come two paragraphs artfully attempting to install the proposers as blood brothers of the taxonomists. Next comes a "unanimous" endorsement of a *principle* (italics theirs) — which cannot possibly be separated from *practice* (italics mine) — that long-unused names be outlawed. There is no indication of how we are to separate sheep from goats, or to how much of the plant kingdom the "principle" is to be applied, and if not to all, how one agrees on a stopping point. Paragraph 5 is a seemingly innocuous suggestion to revive a committee on the nomenclature of economic plants, *provided suitable personnel can be obtained* (I dismiss for the present the question of whether such a committee can legitimately be established, regardless of personnel. I believe not. Cultivated plants do offer special problems both taxonomic and nomenclatural; economic plants which are wild do not.) Following this is an extraordinary paragraph describing two alternatives as if no others were possible. It is an affront to be handed so blatant a piece of specious argument. Lastly, and in view of the past history of American botany, most astonishingly, comes a bald proposal (despite the confused wording that is what is intended) for the special conservation of species names for four American trees, the first hyperbolically described as "the most important timber tree in the United States, if not in the world." (Not only is the resolution not science, here it is simply chamber-of-commerce advertising.) During the disgraceful period (ca. 1885-1930) when the American Code was being spread around (much more by political maneuvers than through intrinsic merit), names of many of our most common plants, trees as well as others, underwent violent changes, often in both parts of the binomials, and this was extended, fantastically, even to vernacular names. Among the staunch supporters of the reckless name-juggling that went on were the professional foresters, primarily those in the employ of the U.S. government. Reading the resolution, one would think that history had never transpired. How is it possible, after all that their organizations countenanced and even promoted until quite recent times, for the representatives of private and governmental American foresters to make such a to-do over a mere handful of Latin names? It would be amazing even if they presented to us a restrained, reasoned, and perceptive slate of suggestions showing due concern for all phases of plant work. It is inexcusable when they present us with a piece of shallow special-pleading. Such a

gross display of hyper-sensitivity and selfishness is unworthy of serious attention.

No society of foresters and no other professional group has produced anything in science comparable to what botanists — professional and amateur — have done. I denounce any proposal to set the interests of any such group above ours. Their very existence would have been impossible without the previous work of the much abused, much imposed upon, much belittled taxonomic botanists. Our science belongs to us, and its rules are for us to decide. We can well be proud of the laboriously, ably constructed systems under which we work. They are the ripened product of the thought and experience of generations of capable devotees. Their failures can more often be laid to shortage of personnel, lack of sufficient time, and difficulties inherent in the materials with which we have to deal than to the rules themselves. They are after all an amazingly workable tool for the extraordinarily intricate and difficult subject in which they are used. Taxonomy is above all a laborious pursuit. To sift through millions of names, and at the same time to better our knowledge of the hundreds of thousands of species involved, is not easy and is not done speedily. We do not arrive at Nirvana ("the absence of striving") in a decade. We shall work better and faster if we do not let ourselves be perpetually distracted and interfered with by those who have only special and selfish interests to consider.

Perhaps worse than the menaces of business men or bureaucrats is the irresponsible eagerness in our own midst to regiment and be regimented by peremptory fiats, in the style exemplified by Dr. Camp's remarks. What he proposes is the abrogation of legal principles. It is my fervent hope that he will not be heeded. His article is neither scientific nor of any help for science. It is to me a shameful thing that taxonomists should rush so eagerly to cater to the "practical" man, forsaking their primary duty to make calm and detached appraisal, and to disregard partisan, unscientific interests. When we do so we are no longer scientists, and deserve neither the attention nor the respect of those who are.

PRIMARY CLASSIFICATION IN THE ORCHIDACEAE
A CRITIQUE

by

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On November 15, 1954, in the 4th fascicle of the 4th volume of Ceiba, there was published an article by Edwin D. Hatch of Central Auckland, New Zealand, entitled "Primary Classification in the Orchidaceae" (reprinted, apparently without change, in the American Orchid Society Bulletin vol. 24, no. 12, pp. 810-811, Dec. 1955). This purported to be a revision of Schlechter's classification of the Orchidaceae to the extent that its nomenclature would "conform to the Code [International Code of Botanical Nomenclature, ed. 1952] without losing sight of the sound principles upon which the original classification was based."

A competent revision of an existing scheme of classification is praiseworthy when carried out in conjunction with a broad systematic treatment of the family. Its publication by itself, as a purely nomenclatural study, would be commendable if it contained a critical discussion of the nomenclatural principles involved, as well as the particular problems inherent in the special group under discussion. Unfortunately, the discussion mentioned above is neither of these. Indeed, as it stands, it serves no practical purpose, and because it does not conform to the requirements of the Code which it professes to support, the names proposed in it have no valid standing.

One hundred years ago J. D. Hooker admonished his contemporaries

> "..that it is only after much preliminary study, and with the aids of a complete library, ..that descriptive botany can be effectively carried out; and it would be well for science if this were fully understood and acted upon."

This is as true today as it was in 1855, and it is as true of purely nomenclatural studies as it is for taxonomic ones.

The Code of Botanical Nomenclature provides strict requirements for precision in nomenclatural matters. Establishment of a legitimate name calls for something more than etymological juggling. Due regard must be given to the historical aspects of any question for the Code (Art. 5) states that

> "in the absence of a relevant rule, or where the consequences of rules are doubtful, established custom must be followed."

Particularly in discussions of suprageneric groupings are such careful studies essential to determine the consequences of the application of the rules when applied to these categories.

Hatch's article is considered to be a poor example of nomenclatural study for three reasons. In the first place, the names which he proposed were not validly published, according to Arts. 42, 43 (2), and 47, and hence have no standing in botanical nomenclature. In the second place, six of the names he proposed would have been illegitimate (Art. 10) even if they had been validly published (Arts. 28, 29). In the third place, the article only serves to compound confusion, since he has ignored the historical basis of the problem.

Hatch, however, does point out a series of problems which were first created by the Paris Congress of 1867. These are the nomenclatural problems dealing with suprageneric taxa (Laws, Art. 8, 21, 22, 23, 24), categories not usually considered by any but monographers or systematists. In general, students have followed the provisions of the rules, but in certain families, such as the Euphorbiaceae and Orchidaceae, where the suprageneric system is complex — and old — the nomenclature is indeed out of line with current requirements.

Unfortunately, the present Code does not provide for the recognition of nomenclatural priority in categories other than genera and species. Articles 28, 29, 32, 35, and 80 establish artificial and arbitrary procedures which run contrary to the stipulations of Articles 26 and 72. The establishment of "type genera" for suprageneric categories must in many cases be a highly artificial procedure. Further, the rules for citation of authority as they apply to suprageneric categories do not necessarily allow for the citation of the earliest authority for a name, nor do they necessarily allow for citation of those careful students who may have put the nomenclature of a particular group on a sound footing.

In the Orchidaceae, the strict application of the Code as it now stands would necessitate the overthrow of suprageneric names familiar to students of the family for over one hundred and fifty years. Ordinarily, one would expect that nomenclatural proposals should accompany a taxonomic revision of the group in question. Since, however, Hatch's article has aggravated the problem in this family, it seems wise to d scuss the names involved, and to ensure stability by proposing certain of them for conservation, as provided by Article 24 of the Code.

At the Amsterdam Congress in 1935, Sprague and Lanjouw presented a list of names of Families which they felt might need to be conserved. Three additional lists of names were added to this at the Stockholm Congress of 1950. Finally, Stafleu, in a preliminary report on the Paris Congress of 1954, stated that the old list (the list of the four combined proposals that had been published in the last edition of the Code) had been thrown out, and that a special committee had been appointed to look into the

formation of a new list of family names which needed to be conserved. He further stated: "When the rules of priority are applied strictly to family names a large number of our commonly accepted names are found to be incorrect." One suspects, however, that fewer names would be changed if the rule of priority were followed than is necessary if Article 28 be applied, since the necessity for determining a type genus from which a family may take its name, or determining that the present family name was taken from a *bona fide* type genus will doubtless present problems in many families. For example, has Solanum ever been specifically and legitimately designated as the type genus of the family Solanaceae? De Jussieu certainly did not designate it, nor did Adanson.

Ames has pointed out that the name "Orchidaceae" was incorrectly formed. The Greek word "orchis" which has the genitive "orchidos" refers to a kind of wild olive, not to the "European plant with testiculate tubers, . ." which Theophrastus called *orchis*. Ames went on to say: ". . an established name in botanical nomenclature, simply because it is philologically faulty, should not be changed." This is, of course, in accord with Article 82. But this does not solve all of our problems in this case.

We are told by Article 28 that "the name of a family is. . taken from the name of its type genus or from a synonym. ." Ames pointed out that it is probable that the plants which originally bore the name "Orchis" were indeed members of the Linnaean genus Orchis of the "Species Plantarum" ed. 1 (1753). Linnaeus in the "Genera Plantarum" and in the "Species Plantarum" did not establish families, but in the "Philosophia Botanica" of 1751 he published (pp. 27-36) a "Methodi Naturalis Fragmenta. ." Here, genera believed to be related were grouped together. The classificatory term of his groups was not given, but the groups were given names.

The group he named "Orchideae" (taken apparently from Hallier's "Enumeratio methodica stirpium Helvetiae indigenarum," 1742) consisted of the 10 genera of orchids (Orchis at the head of the list) known to Linnaeus. Inasmuch as there is no circumscription, or reference to one, this name is not available to us even though there is a Vienna reprint of the "Philosophia Botanica" dated 1755. This is unfortunate, since this name was in common use for the group through the early part of the 19th Century — and, indeed, was used in the Genera Plantarum of Bentham and Hooker as late as 1883.

The first name available to us, which is designated by term, name, and diagnosis, is that of Michael Adanson in his "Familles des Plantes" volume 2, pp. 68-70. Here we are supplied with the name "Orchides." This is probably based on the Orchides of Bernard de Jussieu, published in "Ordines naturales in Ludovici XV horto Trianonensi dispositi" of 1759. I have not seen this work and cannot affirm that the name was validly and legitimately published in it. The one difficulty is that Adanson did not designate a type genus, and while he did include Orchis, it is sixth on his list of included genera. For that matter, I do not know of any statement by any author, until 1861, (Lemaire, L'Illustration Horticole sub t. 277) that designates the genus Orchis as the type genus of the family. However, I think that it will have to be assumed that such is the case and Adanson's name will have to be taken up, with an appropriate change in termination for the name of this family.

If this reasoning be followed it will not be necessary to include the name "Orchidaceae" in any future list of Nomina Familiarum Conservanda, the citation which establishes the priority of the name being:

Orchidaceae, Adanson, Familles des Plantes, vol. 2, pp. 68-70, 1763.

The suprageneric classification of the Orchidaceae has been attempted by a number of students since the time of Linnaeus. Because the family is large, and is complex in floral and vegetative structure, and because a variety of experiences and philosophical

concepts is reflected in these classifications, the various solutions offered to the problems of taxonomy and nomenclature have been diverse. Complicated as the problem is in itself, additional nomenclatural problems have been added by the successive editions of the "Rules [or Code] of Nomenclature." These Rules have, from the first, emphasized the necessity for following the doctrine of priority with regard to the names of genera and species but they have not advocated this for other categories.

In many of the families which have been monographed in recent years, the suprageneric taxa have been named in accordance with the provisions of the Rules. Other families, however (the Orchidaceae and Euphorbiaceae are examples), have not been brought into line. A review and analysis of the situation seems to be in order.

Article 26 of the Code states, "The Rules of priority and typification do not apply to names of taxa above the rank of order." This would seem to be explicit, and by implication the names of taxa below the rank of order should be governed by the Rules of Priority. However, articles 27, 28, and 29 establish an arbitrary procedure for the nomenclature of all suprageneric categories. Similar articles in earlier editions of the Rules created so much confusion, however, that at the Congress in Stockholm in 1950 a list of about 213 family names was proposed for conservation. This proposal was not acted on at that time, and at the Paris Congress (1954), the list was thrown out, with the direction that a new list be drawn up.

The situation with regard to the infrafamilial suprageneric taxa is much more confused. In some families we have names which may be illegitimate, but which are morphologically appropriate and descriptive and have the sanction of priority. It seems ridiculous that names which have been in use by competent students for over one hundred years should be made illegitimate by a legalistic whim. Therefore, it becomes necessary to propose for conservation names in several categories within the Orchidaceae.

In 1877, in his "Entwurf einer natürlichen Anordnung der Orchideen," Dr. Ernst Hugo Heinrich Pfitzer, Professor of Botany in the University of Heidelberg, published a systematic revision of the Orchidaceae. This, and his treatment of the group in Engler & Prantl's "Die natürlichen Pflanzenfamilien," have served as the basis for our modern conception of the classification of the family. Much of Pfitzer's classification was new, but in his primary division he followed the system proposed by Olof Swartz in 1800 which divided the family into two distinct subfamilies, based on the number of fertile anthers. This was in strong contrast to the practice of the majority of orchid systematists of the 19th century, who had prefered to divide the family into a number of tribes.

Swartz, well known as an early student of the Caribbean flora, was also a systematist of note. His "Synopsis Filicum" of 1806 was of considerable value in its time, and his "Afhandling om Orchidernes Slågter och deras Systematiska indelning" is of interest as probably the first attempt to classify the orchids in a natural (but not evolutionary) system. Swartz divided the genera of the Orchidaceae into two groups: "I. Orchider med en Anthera" (orchids with one anther) and "II. Orchider med tva Antherer" (orchids with two anthers). In 1805, in the first volume of Schrader's "Neues Journal für die Botanik," he latinized these to "Orchideae Monantherae" and "Orchideae Diantherae."

In 1887 Pfitzer took up Swartz's taxa, made subfamilies of them, called them Monandrae and Diandrae, and credited the names to Swartz. But these are not the names Swartz used. They are, instead, probably based on the names "Monandria" and "Diandria" used by Willdenow in the 4th edition of the "Species Plantarum" (vol. 4, part 1, p. 8-146, 1805). Since neither Swartz nor Willdenow defined his taxa, the names are illegitimate (Code, Art. 42). However, in 1831 K. S. Kunth, in the

"Handbuch der Botanik," used and defined the subfamilial names "Monandrae" and "Diandrae." Robert Brown and Humboldt, Bonpland, and Kunth had used the name Monandrae at an earlier date, but in both cases for more than one equivalent category and more as a descriptive word than as a term of classification. Therefore, the names should be credited to Kunth and the work cited above.

These names are illegitimate (Art. 29). Despite the fact that: (1) they are descriptive of a fundamental morphological division in the family, (2) can be followed back with a constant usage for 150 years; (3) both modern monographers of the family have chosen to use the names, and (4) we have a Rule of Priority for the names of genera and species, we must, if we would follow the Code, reject these names for a later synonym of one taxon, and a new name which we must coin for the other — or, go through the legalistic maneuver of proposing long established names for conservation.

In the discussion above of the family name, I have alluded to the doubt which must always surround the typification of that name. This doubt is compounded because I am faced with the necessity of naming a type genus for the other suprageneric taxa, particularly since it becomes necessary to overthrow names of long standing. According to the Code, if "Orchidaceae" is accepted for the name of the family, then one must, by definition, accept Orchis as its type genus. If a subfamilial division is required, then one of the subfamilies must bear the name "Orchidoideae." But this is a name which, to my knowledge, has never been used, nor yet been proposed. If proposed it must correspond exactly to Pfitzer's Monandrae, a taxon which uses Kunth's name. Since the name is well circumscribed, and used in all modern works, it seems invidious to reject it as illegitimate. Therefore, I propose for conservation the subfamilial name Monandroideae, Kunth, Handbuch der Botanik, p. 291, 1831.

The second subfamily is not difficult of typification, for Swartz's name, Diantherae, was based on the single genus Cypripedium. In 1817 Kurt Sprengel, in his "Anleitung zur Kenntniss der Gewächse," vol. 2. pt. 1, p. 298, published the "Ordo Cypripedien." This appears, however, to represent a tribe. The first undoubted usage of the name in the rank of subfamily appears to be that by Lindley in the "Orchidearum Sceletos" of 1826. I have not seen this, but I have seen what purports to be a verbatim reprint in "Linnaea," vol. 2, pp. 527-532. Lindley used the name "Cypripedieae," and provided a brief, but probably adequate, diagnosis. He did not give the category a name, but distinguished it by a typographical mark and included within it a category that was designated as "Tribus." However, Lindley did not accept his own designation, for in the "Genera and Species of Orchidaceous Plants," of 1830-40, he converted these to a number of tribes without intercalation of subfamilies. Though Lindley's name is legitimate, it has never been taken up. It seems appropriate, therefore, to conserve Kunth's name which may be cited as: Diandroideae, Kunth, Handbuch der Botanik, p. 292, 1831.

The next taxa which concern us are those that Schlechter termed "Divisio." Unfortunately, this term is preoccupied for the category of Division, a primary taxon of the plant Kingdom. Hatch proposed the term "anther-type" which, besides being unwieldy, was undefined and unlatinized, and hence is illegitimate. For this rank I propose the term "supertribe" (supertribus) which may be defined as a taxon intermediate in rank between subfamily and tribe. This rank has been used before, without a term, by Diels in "A. Engler's Syllabus der Pfanzenfamilien," 11th edition, 1936, e.g., in the Sapindaceae and Acanthaceae. Following Diels, I propose use of the termination *-ae*.

Pfitzer divided his subfamily Monandrae into two supertribes, Basitonae and Acrotonae. These divisions and names were accepted by Schlechter, who noted, however, that the Basitonae corresponded to Lindley's tribe Ophrydeae. (Orchidearum

Sceletos, 1826). Since a name of a taxon has no priority outside its rank (Art. 70), this need not concern us. Inasmuch as this supertribe contains the supposed type genus, the name required by the Code would be Orchidae. This name, however, has never been proposed. For this reason, I propose for conservation the name Basitonae, Pfitzer, "Entwurf einer natürlichen Anordnung der Orchideen," p. 95-96, 1887.

It is even more difficult to determine the typification for Pfitzer's second supertribe, "Acrotonae." No type was designated at its inception and thus we have only mechanical methods for determining what Pfitzer may have had in mind. Possibly the oldest genus might be selected. In this case the type genus would probably be Epidendrum L. Since one tribe of the supertribe is based in part on Lindley's tribe Neottieae, which in turn is based on Robert Brown's Sect. II. Monandrae, one might take the first genus listed there, Thelymitra, as the type genus; or perhaps we should take the first genus listed by Lindley in the initial enumeration of his tribe — Pelexia. Another choice could be the genus from which Lindley took the name of his tribe, Neottia. Thus, the Code allows this supertribe to have at least 4 legitimate names. If, however, the rule of priority is followed, there can be only one legitimate name: Acrotonae, Pfitzer, "Entwurf einer natürlichen Anordnung der Orchideen," p. 96-97, 1887.

At the tribal level, the supertribe Basitonae contains but a single tribe. According to the Code, the name of this tribe should be "Orchideae." This name, as with the similar supertribal and subtribal designations, has apparently never been proposed. Probably the earliest name available is "Granulosae" of Blume, published in the 8th fascicle of his "Bijdragen tot de Flora van Nederlandsch Indië" in 1825. This has apparently never been taken up. Instead, the name "Ophrydeae," proposed by Lindley in the "Orchidearum Sceletos" of 1826, has been almost universally applied. Further, until the present (1952) edition of the Code, this name was legitimate.

As recently as the last edition (1935) of the Rules, botanists were authorized to form the names of families, subfamilies, tribes and subtribes from the name of "..one of the genera in the group.." (Art. 24, 1935). The new edition of the Code, with its explicit directions for the formation of names makes much previous work illegitimate. This, despite the warning of Article 4 of the present Code, "The essential points in nomenclature are: (1) to aim at fixity of names; (2) to avoid or to reject the use of forms and names which may cause error or ambiguity or throw science into confusion. Next in importance is the avoidance of all useless creation of names.." I submit that the present reading of Articles 28 and 29, as evidenced by the problems presented in this paper, do not promote fixity of nomenclature, does tend to throw science into confusion and does promote useless creation of names.

Despite the fact that it is not the earliest name available, but because it was legitimate for 85 years, and because it has been almost universally used since its first publication in 1826, I propose for conservation the tribal name Ophrydeae, Lindley, "Orchidearum Sceletos," 1826, for this taxon.

The supertribe Acrotonae of Schlechter's classification was divided into 2 tribes, which he called "Polychondreae" and "Kerosphaereae." The "Polychondreae" corresponds almost exactly with the tribe "Neottieae" of Bentham, which in turn was made up of the "Neottieae" and "Arthuseae" of Lindley. Though Lindley's Neotteae appears to be a synonym of Blume's Pulvereae, it seems logical to conserve the name which has had more than a century of use, and which was legitimate until 1950. Therefore, I propose for conservation the tribal name Neottieae, Lindley, "Orchidearum Sceletos," 1826, emend. Bentham in Bentham and Hooker f., "Genera Plantarum," vol. 3, part 2, p. 480, 1883.

Schlechter's tribe "Kerosphaereae" is made up of the Tribes Epidendreae, Lindley,

and Vandeae, Lindley. On grounds of priority, the name Epidendreae, Lindley is the legitimate name. It is here, however, that the attempt to revise nomenclature without revising the system breaks down. Schlechter divided his tribe into two "series," and the "series" into subtribes. These subtribes seem to be equivalent to the subtribes in the rest of the family. If this is true, then the "Series" should be treated as tribes and the "Tribe" as a supertribe. But this, it seems to me, takes us out of nomenclature into systematics, and here it is unwise for me to tread. Therefore, I leave the nomenclature, as well as the systematics, of this last group of the Orchidaceae to a student of the group to untangle.

The foregoing is a summary of the nomenclatural problems of the Orchidaceae on the suprasubtribal level. The nomenclature of the subtribes as given by Schlechter seems for the most part to be legitimate under the present Code and need not concern us. The higher taxa do present problems and the proposals here presented are an attempt to remedy the difficulties. It is felt that the treatment of the nomenclature of suprageneric taxa is not, and never has been, treated adequately in the Code or the Rules. It is felt that serious thought must be given to revision of Articles 27, 28, and 29, with a view to reenforceing the principle of priority and to stabilizing nomenclature.

Nomina conservanda proposita	Nomina rejicienda proposita
Subfamilial Names	
Monandroideæ, Kunth, Handbuch der Botanik, p. 291, 1831.	
Diandroideæ, Kunth, Handbuch der Botanik, p. 292, 1831.	*Cypripedoideæ*, Lindley, Orchidearum Sceletos, p. 7, 1826 [ieae, Lindl.].
Tribal Names	
Ophrydeæ, Lindley, Orchidearum Sceletos, p. 7, 1826.	*Granulosæ*, Blume, Bijdragen tot de Flora van Nederlandsch Indië I : 402, 1825.
Neottieæ, Lindley, Orchidearum Sceletos, p. 7, 1826, emend. Bentham in Bentham and Hooker f., Genera Plantarum, 3(2) : 480, 1883.	*Pulvereæ*, Blume, Bijdragen tot de Flora van Nederlandsch Indië 1 : 417, 1825.

Nomina proposita

Supertribus

Basitonæ, Pfitzer, Entwurf einer natürlichen Anordnung der Orchideen, p. 95-96. 1887.
Acrotonæ, Pfitzer, l.c., p. 96.

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ON THE APPLICATION OF THE NAME OURAGOGA

by

A. A. Bullock (Kew)

The history and application of the generic name *Ouragoga* (or *Uragoga*) began in 1737, when it was used by Linné in the first edition of the *Genera Plantarum* (p. 378) and in the *Hortus Cliffortianus* (p. 486). In both works Linné referred to the Ipecacuanha of Marcgraf and Piso, but there is no evidence that he had at that time seen a flowering specimen, since he placed the genus among miscellaneous genera of uncertain position in his system.

The plant is apparently not mentioned, either under the vernacular Ipecacuanha or under any other name in subsequent editions of Linné's *Genera Plantarum* and it is omitted from the *Species Plantarum* (1753). The name *Ouragoga* appeared again in 1774 in Wickman's dissertation "*Viola Ipecacuanha*". He gave a description based upon those of Linné, together with various names and descriptions taken from other authors. Wickman's conclusions in regard to identification are: "Ex omnibus hisce recentiorum descriptionibus patet Ipecacuanham speciem esse *Violae*....," and "Priorem Ipecacuanham Pisonis & Ouragogam N. D. Praesidis [Linné], eandem forte speciem esse caule decumbente probabile videtur, perinde atque eadem Pisonis & Barerii [*sic*] species est foliis villosis. [Ob] Calcar longum corollae in flore Allamandi (calcari longissimo) species sine dubio diversa est a Pombalia Vandellii quae calcari destituitur: praeterea nec Piso, nec Marcgravius calcar nominat aut delineat, quod minime praeterire potuissent, si illud observassent. Atque haec suit causa, cur N. D. Praeses in Syst. Naturae edit. 13. p. 669 *Ipecacuanham* Barreri a *Viola diandra* Allamandi distinxerit."

It is clear that Wickman had no intention of maintaining *Ouragoga* as a generic name; he merely repeated Linné's description in order to show that the plant concerned was in fact a species of "*Viola*". Linné himself referred "Ipecacuanha alba *officinis*. Viola grandiflora, veronicae folio villoso. *Barrer. aequin.* 113* vide descriptionem". to *Viola ipecacuanha* in the Mantissa Altera, p. 484 (1771) and this is the *Hybanthus ipecacuanha* (Linn.) Baill. of later authors. Wickman evidently regarded the *Ouragoga* of the *Hortus Cliffortianus* and the *Genera Plantarum* (ed. 1) as a synonym, though he was not sure of the specific identification. Under the International Rules *Ouragoga* is not, therefore, a validly published name.

In the *Flora of Tropical Africa* (3 : 223. 1871), Hiern remarked under *Cephaelis* Sw. that "According to the usual principles of nomenclature, one of the names of Aublet, quoted by Bentham and Hooker, ought to be employed for this genus in preference to Swartz's; a still older name, however, is *Ouragoga* Linn. Gen. Pl. edit. i., p. 378 (1737)". Later, Hiern actually adopted the name *Ouragoga* in preference to *Cephaelis* (in Cat. Afr. Pl. Welw. 1 : 498 (1898), where he cited Wickman's dissertation).

Hiern's suggestion was taken up by Baillon in 1878 when in his "Adansonia" (12 : 222-230. 1878-9), he published ten new species under the generic name *Uragoga*, an orthographic variation of *Ouragoga*. In 1879, in the same volume, he published his "Mémoire sur les *Uragoga*" in which he included a great number of species currently referred to *Psychotria, Grumilea, Cephaelis, Geophila* and other genera. In all he named forty-two sections, many of which are usually given generic status. From the point of view of typification it is sufficient to say that he regarded *Uragoga* as the correct name (by priority) for *Cephaelis* Sw. He noted incidentally that *Tapogomea* Aubl. was also an earlier synonym of *Cephaelis* Sw.

In his "Histoire des Plantes" (7 : 280. 1880), Baillon makes quite clear that his *Uragoga* is the same as Linné's *Ouragoga;* he also makes quite clear that it is based upon "La plante qui produit l'Ipecacuanha ordinaire [C'est à dire l'*Ipecacuanha annelé mineur*]. . ." To this plant he applied the name *Uragoga ipecacuanha* and it is undoubtedly the plant now more usually known as *Cephaelis ipecacuanha*, (Brot.) A. Rich. not, as suggested below, necessarily the ipecacuanha named *Ouragoga* by Linné! In the same work he discussed the "Série des *Uragoga*", but did not make sufficiently clear which of the names he used were to be regarded as generic and which as infrageneric.

K. Schumann in Engl. et Prantl (Natürl. Pflanzenfam. 4 (4) : 119. 1891), adopted Baillon's spelling *Uragoga* but attributed the name to Linné, quoting *Cephaelis* Sw., *Tapogomea* Aubl., *Carapichea* Aubl., *Evea* Aubl., *Eurhotia* Neck., *Callicocca* Schreb., *Suteria* DC. and *Camptopus* Hook. f. as synonyms. In 1873 J. D. Hooker's account of the *Rubiaceae* in the *Genera Plantarum* of Bentham and Hooker had appeared. He did not mention *Ouragoga*, but cited *Tapogomea, Carapichea, Evea, Eurhotia* and *Callicocca* as synonyms of *Cephaelis* Sw. (vol. 2, p. 127). O. Kuntze (Rev. Gen. 298. 1891) also adopted the name.

More recently, the generic name *Uragoga* has been adopted for many African species by De Wildeman ("Remarques à propos de formes du genre *Uragoga* L." in Mém. Inst. Roy. Colon. Belge, 4. 1936) and by Schnell ("Contribution à l'étude des *Uragoga* de l'Ouest africain" in Bull. Inst. franç. d'Afr. noire, 15 : 98-132. 1953). These authors were well aware of the synonymy of *Uragoga* and *Cephaelis*, but they did not, apparently, observe that the name is illegitimate, its valid publication dating only from 1879 (Baillon).

Swartz published the conserved generic name *Cephaelis* in his "*Nova genera & species seu prodromus descriptionem vegetabilium*" (45. 1788), and under it he placed four species, *C. violacea* (based upon *Tapogomea violacea* Aubl.), *C. elata* (new species), *C. axillaris* (new species) and *C. muscosa* (based upon *Morinda muscosa* Jacq.). He characterized the genus as "*Flores* capitati involucrati. *Corolla* tubulosa. *Stigma* 2-partitum. *Bacca* bisperma. *Receptaculum* paleaceum (inter *Morindam* & *Conocarpum*)".

Tapogomea Aubl. is rejected in favour of *Cephaelis* Sw. so long as the two names are regarded as taxonomic synonyms; it is unfortunate that *C. muscosa* (Jacq.) Sw. has been chosen as the lectotype species. The choice of *C. violacea* (Aubl.) Sw. would have ensured the permanent rejection of *Tapogomea* as a nomenclatural synonym.

It will be observed that no author since Wickman (1774) has critically examined the evidence for regarding *Ouragoga* [Linn.] as a synonym of *Cephaelis* Sw. Linné's citation of the vernacular "Ipecacuanha" appears to have led directly to the assumption that he was referring to the plant now known as *Cephaelis ipecacuanha*, to the neglect of the strong evidence (since Wickman's paper must have been approved by Linné himself) that it was in fact *Hybanthus ipecacuanha*.

NACHSCHRIFT ZUR WORTFORM TAXONOMIE

von

J. Paclt (Bratislava)

Unlängst habe ich gezeigt (Taxon 4 : 177-178. 1955), dass die Einsprüche bestimmter Autoren gegen die Existenzberechtigung der Wortformen "Taxonomie" und „Taxon" als unbegründet angesehen werden müssen. Inzwischen haben verschiedene Forscher, darunter F. Widder - Graz, ihre volle Zustimmung zu den Schlussfolgerungen meines Artikels ausgesprochen.

An dieser Stelle mögen indessen einige Bemerkungen über den Erfinder des Terminus "Taxonomie" folgen. In meinem oben zitierten Artikel habe ich noch Duby 1832 als den ältesten bekannten Benutzer dieses Terminus angeführt. Kurz darauf ist aber eine Mitteilung erschienen (Hölder 1955a), in welcher der Nachweis geliefert wird, dass die Priorität dem Botaniker Candolle "1819" gebührt. Candolle, der eigentliche Erfinder des Begriffes, benutzt wieder nur die Schreibweise "Taxonomie" (weder "Taxinomie" noch "Taxionomie"). Die erste Auflage der Candolleschen Schrift stammt allerdings nicht aus dem Jahre 1819, sondern 1813. Nach einer Revision der Auflage von 1813 steht nun wohl endgültig fest, dass der Schöpfer des Begriffes "Taxonomie" Candolle 1813 ist.

Candolle sagt nichts Näheres über die Etymologie seines Terminus. Das mediävale Latein kennt jedoch eine "vox barbara", nämlich taxo, taxare (vgl. Wagner 1729), die Candolle gegebenenfalls unmittelbar als Ableitungsquelle gedient haben dürfte.

Der Artikel von Hölder 1955a macht den Eindruck, als ob der Verfasser dem verbessernden Vorschläge Richters folgen möchte. Aus einer anderen Veröffentlichung desselben Autors (Hölder 1955b) geht jedoch hervor, dass es nicht der Fall ist. Die Schreibweisen "Taxionomie" bzw. "Taxinomie" finden (vor Richter 1954) m.W. nur bei den folgenden Autoren Platz: Leunis 1860, Costa Lima 1938, Pagés 1952 und Orfila 1954.

Vor kurzem wurde endlich für den Forschungszweig der Ethologie (Verhaltensforschung, behaviourism) ein überflüssiger Terminus novus, nämlich Taxiologie, vorgeschlagen (Richter 1955). Uns interessiert hier dieser Vorschlag nur von der terminologischen Seite. Richter schreibt: "Die Aehnlichkeit von Taxiologie mit Taxionomie [=Taxonomie] würde ebensowenig stören wie bei Oekologie, Oekonomie oder Geologie/Geographie". Mit dieser Bemerkung würde ich mich einverstanden erklären, wenn nicht der Richtersche Terminus novus bereits durch den Begriff "Taxilogie" Panchich 1842 vergeben wäre. Nach einer freundlichen Mitteilung des Grazer Botanikers Prof. Felix Widder, der mich auf diese seltene Schrift so dankenswert aufmerksam gemacht hat, benutzte Panchich 1842 lediglich die Schreibweise "Taxilogie", wobei er mit diesem Terminus unsere "Taxonomie" verstanden hat.

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INTERNATIONAL ORGANIZATION OF PALAEOBOTANY (I.O.P.)

Dans l'intervalle du 8° Congrès international de Botanique de Paris et du 9° Congrès international de Botanique qui doit tenir ses assises à Montréal (Canada), l'I.O.P. s'est fixé un programme minimum d'activité. Les publications qui en résultent seront adressées régulièrement aux membres de l'I.O.P. qui ont leur cotisation payée.

1955. Premier "*World Report on Palaeobotany*".

Il s'agit d'une Bibliographie internationale des années 1950-1954 comprenant un grand nombre de références numérotées.

Ce travail est actuellement sous presse.

1956. Second "*World Report on Palaeobotany*".

Il comprend:

a. un Complément bibliographique du prémier "World Report on Palaeobotany" où figureront les références omises l'année précédente.

b. un Index des Paléobotanistes mondiaux avec leurs adresses.

1957. Troisième "*World Report on Palaeobotany*".

Bibliographie internationale des années 1955-1956.

1958. Quatrième "*World Report on Palaeobotany*".

Bibliographie internationale de l'année 1957.

Ces travaux seront conduits à bonne fin, en grande partie, grâce à l'activité des correspondants de l'I.O.P. et grâce à la compréhension des Paléobotanistes de tous pays qui font parvenir en temps voulu les tirés à part et la liste de leurs travaux. Il est recommandé aux auteurs d'accompagner leurs références bibliographiques d'une courte analyse comportant obligatoirement la liste des espèces nouvelles.

Les recueils bibliographiques devront contenir *tous* les travaux effectivement publiés. Il est souhaitable que leur publication soit connue le plus tôt possible. Si des oublis ont lieu dans les bibliographies déjà publiées, les auteurs sont invités à faire connaître d'urgence le titre des références omises qui sera ajouté au Report suivant.

Rudolf Florin
Président de l'I.O.P.

Édouard Boureau
Secrétaire général de l'I.O.P.
61 Rue Buffon
Paris-Ve, France

News and Notes

CARL OTTO ROSENDAHL (1875-1956)

We regret to announce that Dr Carl Otto Rosendahl died on March 4, 1956. Dr C. O. Rosendahl was Professor of Botany emeritus at the University of Minnesota and a well-known authority on the recent and fossil flora of Minnesota as well as on Saxifragaceae.

CORNELIUS LOTT SHEAR (1865-1956)

The death is announced of Dr Cornelius L. Shear, lately of Monroe, Louisiana. Dr Shear died on February 2, 1956; he was a former Head of the Division of Mycology and Disease Survey, Plant Industry Station, Beltsville, Maryland, U.S.A. His extensive mycological collections are preserved at Beltsville (BPI). From his manifold activities in the field of Mycology we recall here as an example his work on the continuation of Ellis and Everhart's *Fungi Columbiani*, which series of exsiccatae was edited by him from 1903 until 1907. In 1895 he edited a series of exsiccatae known as *'New York Fungi"* (4 centuries); his own collections were made all over the United States and Cuba.

A REMARKABLE PUBLICATION

The International Council of Museums has had the excellent idea to appoint a committee "charged with the task of considering all means of impressing on such museums (i.e. natural history museums) the importance of type specimens." The results of the work of the committee have been published by its chairman, Dr W. E. Swinton, and the publication, named "Type specimens in botany and zoology", has been subventioned by UNESCO. *)

It is a curious document. Already on p. 1 we are informed that Linnaeus inaugurated binary nomenclature "in a great work which he published in 1758". (Species plantarum ed. 1 was published in 1753). A few lines further down the author, after having mentioned the Rules of Zoological nomenclature, continues: "In the case of plants the rules are said not to be quite so well codified". It is rather astonishing to find reference to and evaluation of the Rules of Botanical Nomenclature based upon hearsay. However, we are immediately comforted that the botanical rules are "subject to revision and amplification at the triennial meetings of the International Botanical Congresses" (the interval between the congresses is five years except for Stockholm-Paris, which was four). The remarkable p. 1 winds up with the statement that for every species described "there must originally have been a type-specimen". Most species described till now have had no type specimens.

On p. 2 one is slightly amused by the reference to the "national museums of England, Scotland, France and the United States" as the main repositories of type specimens.

But p. 3 again contains remarkable statements: "All ... biologists in the course of description of a new genus or species of an animal ... must designate" a type specimen. Not a word about provisions in botany. And further: "This specimen is known as the type-specimen of the particular genus or species". (A type of a genus is a species, not a specimen). The sole definition of a type as "a specimen which... most satisfactorily demonstrates the characters on which the species is based", is also open to discussion, to say the least.

Even if the concentration of inaccuracies is considerably lower in the following pages, one must regret that a publication which has been launched under such auspices, is so unsatisfactory. One must recommend to the bodies concerned to take the only possible action: withdraw the publication.

K. Faegri.

*) Swinton, W. E., Type specimens in botany and zoology. Recommendations for their conservation in natural history and general museums. — Paris. International Council of Museums. 1955. 11 p.

NOMINA CONSERVANDA PROPOSITA

(11) Proposal for the conservation of the generic name **Santaloides** Schellenb. (1910) of the *Connaraceae*, versus *Santalodes* O. Ktze. Rev. Gen. 1 : 155 (1891), of the *Connaraceae*.

Santaloides Schellenb. Beitr. Vergl. Anat. Syst. Connar. in Mitt. Bot. Mus. Univ. Zürich 50 : 38 (1910), in clavi, et in Engl. Pflanzenr. Connar. [4 (127)] 119 (1938); Hutch. et J. M. Dalz. Fl. W. Trop. Afr. 1 : 516 (1928); Exell et Mendonça, Consp. Fl. Angol. 2:146 (1954); J. H. Hemsley in Turr. et Milne-Redh. Fl. Trop. E. Afr. *Connaraceae*, ined.

Holotype species: *Santaloides minus* (Gaertn.) Schellenb.

Size and area: A genus of some forty-five species, widely distributed in the tropics of the Old World.

Discussion. The generic name *Santaloides* was first used by Linnaeus, Fl. Zeyl. 192 (1747) for a plant from Ceylon, but thereafter the name disappeared from the literature until it was resuscitated by O. Kuntze in 1891 with the spelling deliberately altered to *Santalodes*. Kuntze cited the Linnean work, but also cited *Rourea* Aubl. (1775) as a synonym, thereby rendering his own *Santalodes* illegitimate. Kuntze's name was rightly ignored by botanists for the next twenty years, the name *Rourea* Aubl. being accepted for both Old and New World species. In 1910 Schellenberg recognised that the Old and New World species should be referred to distinct genera. He correctly retained *Rourea* Aubl. for the New World species and accepted Kuntze's name, with the corrected spelling *Santaloides*, for the Old World species. He cited the name in the form "*Santaloides* [Linn.] O. Ktze. emend. Schellenb.", not realising that he was in fact making a later homonym. The argument that "*Santaloides*" and "*Santalodes*", both derived directly from *Santaloides* Linn. (1747), are different names cannot be maintained.

The conservation of *Santaloides* Schellenb. (1910) is therefore proposed in order to avoid a number of name-changes, which would serve no useful purpose.

Proposed by: J. H. Hemsley & A. A. Bullock, Royal Botanic Gardens, Kew, Surrey.

* * *

(12) Proposal for the conservation of the generic name 7990 **Stenandrium** Nees (Acanthaceae) in Lindl. Introd. Nat. Syst. ed. 2. 444 (1836) versus *Gerardia* L. (Acanthaceae) Sp. Pl. 610 (1753).

Stenandrium Nees (Acanthaceae): nom. cons. prop.

Adopted by: Nees, ex De Candolle, Prodromus 11 : 281 (1847); Nees, ex Martius, Fl. Bras. 9 : 74 (1847); Benth. et Hook. fil., Gen. Pl. 2 : 1095 (1876); Baillon, Hist. Pl. 10 : 461 (1891); Lindau, ex Engl. et Prantl, Pflanzenfam. 4. 3B : 320 (1895); Urban, Symb. Ant. 2 : 206 (1900), 4 : 576 (1911), 9 : 129 (1923); Small, Fl. Southeastern U.S., ed. 1 : 1085 (1903), and in ed. 2 : 1085 (1913); Johnston, Contrib. U.S. Natl. Herb. 12 : 110 (1908); Standley, Journ. Arn. Arb. 11 : 48 (1930); Leonard, Carn. Inst. Wash. Publ. 461: 212 (1936), and in Kew Bull. 1938 : 62 (1938), and in Journ. Wash. Acad. Sci. 32 : 187 (1942), and in Contr. U.S. Natl. Herb. 31 : 124 (1953).

Type species: *Stenandrium tuberosum* (L.) Urban.

Gerardia L. (Acanthaceae): nom. rej. prop.

Adopted by: Small, Fl. Miami 168 (1913), and in Man. Southeastern Fl. 1229 (1933); Blake, Contrib. Gray Herb. N.S. 52 : 100 (1917), and in Rhodora 20 : 68 (1918); Britt. et Millsp., Bahama Fl. 402 (1920).

Type species: *Gerardia tuberosa* L.

Discussion: In Species Plantarum (1753) Linnaeus included 5 species under *Gerardia*. The first choice of a lectotype for this genus was made by Smith (in Rees' Cyclopedia 16. 1810) who chose *G. tuberosa*, the first name cited by Linnaeus, and remarked, "... this plant must be the true, though it were the only *Gerardia*, and the rest in that case must have a new generic appellation and character." The choice of *G. tuberosa* as the lectotype was, over a century later, reaffirmed by Pennell in 1913 (Bull. Torr. Bot. Club 40 : 120) and again in 1930 (Proc. Acad. Nat. Sci. Phila. 82 : 19) and 1935 (Monog. Acad. Nat. Phila. 1 : 419). Pennell's conclusions, reached after critical study of Linnaeus' descriptions and of historical evidence, seem incontrovertible. There can be little doubt that the lectotype of *Gerardia* should be *G. tuberosa*.

The realization came early that *G. tuberosa* and the four other species included in *Gerardia* by Linnaeus should not be associated in one genus. Evidently considering *G. tuberosa* as the "true" *Gerardia* and not wishing to propose a new name for the others, Walter in his Flora Caroliniana (1788), reduced *Gerardia* to synonymy and placed three of Linnaeus' other species in one of his many genera called *Anonymos*. Since 1810 no writer seems to have retained *G. tuberosa* in the Scrophulariaceae. Bentham in 1846 (in De Candolle, Prodromus 10 : 520) assigned this species to the Acanthaceae. Earlier, by Sprengel in 1825 (Linn., Syst. Veg., ed. 16. 2 : 825), the name had been treated as a synonym of *Ruellia rupestris* Sw. of the Acanthaceae, which species was later incorporated into the new acanthaceous genus *Stenandrium* by Nees in 1847 (in De Candolle, Prodromus 11 : 283). Urban, in 1911 (Symb. Ant. 4 : 576), made the new combination *Stenandrium tuberosum* (L.) Urb., placing *S. rupestre* (Sw.) Nees in synonymy thereunder. Recently Leonard, the outstanding American student of the Acanthaceae, has remarked that *Gerardia tuberosa* belongs in his pet family (Contr. U. S. Nat. Herb. 31 : 126. 1953).

There seems little doubt that, following strict application of the Code, *Stenandrium* Nees should rightly be treated as a later name and synonym for *Gerardia* L., the correct name for the acanthaceous genus and, unfortunately, a name that has long been mistakenly associated with a genus of the Scrophulariaceae. The perennial misapplication of *Gerardia* to a taxon of the Figwort family was given new life by Miss Green who in 1929 (Prop. Brit. Bot., Int. Bot. Cong., Cambridge 1930 : 167), in her selection of so-called "standard-species" to typify certain Linnean genera, arbitrarily chose *Gerardia purpurea*, Linnaeus' second species, as the lectotype. This selection was quite unnecessary — as the choice of a lectotype had already been indicated by Smith and Pennell — and cannot be maintained since Pennell in 1930 (Proc. Acad. Nat. Sci. Phila. 82 : 19) showed that this species does not fit the original description of *Gerardia* as well as another of the original elements, *G. tuberosa* (see Stockholm Code, Art. 19 and Appendix I, 4e). After pointing out that the name *Gerardia* can no longer be applied to the scrophulariaceous taxon, Pennell demon-

strated that the correct name for the genus is *Agalinis* Raf. Unfortunately and incorrectly the name *Gerardia* L. is still applied to *Agalinis* Raf. by various authors.

If the rules of priority are adhered to, the name *Gerardia*, which must be replaced in the Scrophulariaceae by *Agalinis*, would have to replace, in turn, *Stenandrium* of the Acanthaceae. Such a course would involve many nomenclatural transfers in *Stenandrium*, a genus of more than 60 recognized species (*fide* Leonard, Contrib. U.S. Natl. Herb. 31 : 124. 1953). To avoid these changes it is desirable to conserve the well-known name *Stenandrium* Nees over the so-long-misused *Gerardia* L. for this taxon of tropical American Acanthaceae.

Proposed by: John W. Thieret (Chicago).

TUPARI INDIAN PLANTS

On a recent field trip, supported by the "Fonds National Suisse pour la Recherche Scientifique", the Swiss ethnographer Dr Franz Caspar has been able to gather botanical material when living with a South American tribe of the Amazon basin (Tupari Indians, Rio Guaporé region, about 12° S., 62° W. Gr.). Dr Caspar is no botanist and was principally interested in collecting cultivated species as a contribution to the knowledge of aboriginal agriculture and of the geography of cultivated plants.

Some dried material of cultivated and wild species has been deposited at the Jardim Botânico in Rio de Janeiro and classified as follows (in some cases tentatively in the absence of flowers or fruits):

a) cultivated

Phaseolus sp. (2 species, *Ph. vulgaris* and *lunatus*?)
Dioscorea sp. (edible)
Spilanthes oleracea (medicine)
Phytolacca sp. (*Ph. icosandra*?)

b) wild

Xylopea sp. (wood for making bows)
Periandra dulcis (medical use)
Rubiaceae, *Psychotria* (?) (medical or magic use)
Helicteres sp. (magic use)
Serjania sp. (fish poison)
Derris sp. or *Lonchocarpus sp.* id.
Piperaceae: *Peltobryon* or *Artanthe* id.
Compositae (gen.?) id.

The most interesting material seems to be the collection of living plants (seeds, roots, tubers) of about 30 different species or varieties, mostly species cultivated by the Tupari Indians. The following tentative classification has been given by Dr J. G. Kuhlmann, former director of the Jardim Botânico in Rio de Janeiro, to whom the ethnographer herewith extends his thanks.

a) cultivated

Dioscorea sp. (6 species or varieties, all edible)
Ipomoea sp. (sweet potato, 2 varieties)
Pachyrrhizus bulbosus (edible tubers)
Calathea sp. (edible tubers)
Xanthosoma sp. (?) (edible tubers, 2 varieties)
Arachis hypogaea (8 varieties)
Zea mays (3 varieties)
Phaseolus sp.
Capsicum sp.
Cucurbitaceae (seeds edible)
Zingiber officinale (magic use)
Cleome (*glandulosa*?) (medical use)
Nicotiana tabacum
Gossypium sp.
Oryza subulata (stalk used as cutting tool)
Sclerea sp. (seeds used for ornament)
Coix lacryma id.
Canna sp. id.

b) wild

Chrysophyllum sp. (seeds used for ornaments)
Olyra latifolia id.
Astrocaryum mumbaca (?) id.
Omphalea diandra (Euphorbiaceae) (oil from fruit used for toilet)

These seeds have been planted in the "Jardim Botânico" at Rio de Janeiro, in August 1955, under the direction of Dr José Corrêia Gomes. It has been agreed that specialists in any field covered by the species planted shall be invited to ask for this material and information they may want for their studies. Most of the plants cultivated by the Indians and agricultural procedures have been photographed by Dr Caspar, and prints are available.

Further information is offered by Dr Franz Caspar, Hamburg-Niendorf (Germany), Paul-Sorge-Strasse 27.

BOTANISCHE STAATSSAMMLUNG MÜNCHEN

Dr Hermann Merxmüller, Curator of the scientific collections and Lecturer at the University of Munich has been appointed successor to the late Prof. Dr K. Suessenguth as director of the Botanische Staatssammlung, München.

*

Dr Herman Merxmüller, Konservator an den wissenschaftlichen Sammlungen und Privatdozent an der Universität München, wurde als Nachfolger des verstorbenen Prof. Dr K. Suessenguth mit der Leitung der Botanischen Staatssammlung München betraut.

REQUEST

Professor J. E. Canright, College of Arts and Sciences, Indiana University, Bloomington Indiana requests material of the following groups:

Annonaceae: viable seeds of any of the Asian and/or African species.

Lauraceae: viable seeds and preserved flowers of any species with the exceptions of *Persea americana* and *Sassafras albidum.*

Hernandiaceae: viable seeds of any species of the genera *Hernandia, Illigera, Gyrocarpus,* and *Sparattanthelium.*

Canellaceae: viable seeds, samples of mature wood, and preserved flowers and leaves of *Canella, Cinnamosma, Cinnamodendron, Capsicodendron, Pleodendron,* and *Warburgia.*

If prospective collectors wish to obtain bottles and preservative, Prof. Canright can be contacted at the above address for further arrangements.

Office

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CONTENTS

Ross, R. — Homonyms, nomina nuda, and the Douglas Fir 41
Shinners, L. H. — Pseudotsuga and Pseudo-science 43
DeWolf, G. P., Jr. — Primary classification in the Orchidaceae 46
Bullock, A. A. — On the application of the name Ouragoga 53
Paclt, J. — Nachschrift zur Wortform Taxonomie 55
International Organization of Palaeobotany (I.O.P.) 56
News and Notes 56
Carl Otto Rosendahl (1875-1956) . . 56
Cornelius Lott Shear (1865-1956) . . 56
A remarkable publication (K. Faegri). 57
Nomina conservanda proposita . . . 57
(11) *Santaloides* Schellenb. (1910) . 57
(12) *Stenandrium* Nees (1836) . . 58
Tupari Indian Plants (F. Caspar) . . 59
Botanische Staatssammlung München . 60
Request 60
I.A.P.T. 60
Contents 60

"Taxon" is directed by: Prof. J. Lanjouw, Chief Editor, and Dr F. A. Stafleu, Assistant Editor. All correspondence should be sent to the International Bureau for Plant Taxonomy and Nomenclature, 106 Lange Nieuwstraat, Utrecht — Netherlands.
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